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D'ALEMBERT'S CRITIQUE OF PROBABILITY THEORY

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SUMMARIES

This paper seeks to understand d'Alembert's critique of conventional probability theory in terms of a broader philosophical program aimed at achieving a closer match between mathematics and physical and psychological experience. D'Alembert's discussions of the St. Petersburg and inoculation problems are reinterpreted in light of this philosophical objective.

Dans cet article, nous cherchons à comprendre la critique que fait d'Alembert de la théorie des probabilités conventionnelle, et ceci, dans le cadre d'un programme philosophique élargi ayant pour but une meilleure adéquation entre la mathématique et l'expérience physique et psychologique. La discussion par d'Alembert du paradoxe de St-Petersbourg et du problème de l'inoculation est réinterprétée à la lumière de cet objectif philosophique.

Although probability theory did not receive full analytical expression until Laplace's *Théorie Analytique des Probabilités* (1812), its elements had been established in the work of Pascal, Fermat, De Moivre, Huyghens, and Jacques Bernoulli and widely accepted by the European mathematical community by the first quarter of the eighteenth century. Hence, Jean de la Rond d'Alembert's (1717-1783) sharp and prolonged criticisms of the assumptions and definitions of probability theory have been viewed by most historians of mathematics as the aberrations of an otherwise outstanding mathematician. In this paper, I wish to show that d'Alembert's critique of conventional probability theory, far from being an isolated instance of perversity on the part of an extremely able mathematician, formed part of a much more extensive discussion of the relationships between mathematics and the world of both physical and psychological experience. D'Alembert's alternative formulations of probability theory sought to apply the inductive methods of the physical sciences to the determination of probability values, and to refine ordinary notions of mathematical expectations so as to take account of the so-called "moral" (i.e., social and psychological) complexities of the problems, such as the desirability

of smallpox inoculations, assigned to the theory. Similarly d'Alembert attempted to incorporate standards of physical plausibility into probability theory in order to make it a more effective mathematical tool in the physical sciences. Through such adjustments, d'Alembert hoped to construct a theory of probability more closely modeled on the physical and psychological realities of the situations to which the theory might be applied.

D'Alembert's concern with the philosophical underpinnings of mathematics can be traced to his earliest scientific works, such as the *Traité de Dynamique* (1743), and followed throughout his mathematical career. Almost every one of d'Alembert's scientific memoirs contained philosophical reflections on the nature of mathematical certainty, the origin of mathematical concepts, or the proper method of applying mathematics to natural philosophy. In his double role of mathematician and prominent *philosophe*, d'Alembert united a range of interests and talents which were exceptionally broad, even as judged by the catholic standards of the 18th century. D'Alembert exemplified his own spirited rebuttal to those who accused mathematicians of a narrow, provincial intellect which analyzed everything it touched to dryest dust. D'Alembert's counter-examples (and, it is fair to assume, personal models) were Descartes and Pascal, whose eloquence and philosophical acumen matched their mathematical proficiency [d'Alembert 1757, 627]. For his part, d'Alembert was thoroughly familiar with the works of Descartes, Locke, and Condillac, and synthesized elements from the philosophies of all three into a distinctive view of mathematical knowledge which guided his approach to problems in mechanics, the calculus, and probability theory. A complete appreciation of d'Alembert's work in these areas presupposes an understanding of his philosophy of mathematics.

The Lockean contribution to d'Alembert's philosophy of mathematics is most striking in the "Preliminary Discourse" to the *Encyclopédie*. All human knowledge, d'Alembert asserts, derives from experience. Following Locke, he divides knowledge into two types: direct knowledge, resulting from immediate sensation; and reflected knowledge, produced by the operations of the mind upon these direct ideas [d'Alembert 1963, 6]. As a mathematician, d'Alembert is concerned almost exclusively with the latter, mediated form of knowledge. Borrowing from Descartes [Descartes 1955], d'Alembert assumes analysis to be the fundamental intellectual operation turned upon the raw materials of sensation. Property by property, the mind strips away the tangle of particular features which compose any sensation until it arrives at the barest skeleton or "phantom" [d'Alembert 1963, 19] of the object, shaped extension.

It is at this rarefied level that mathematics studies the objects of experience. Although these objects have been sys-

tematically denuded of all those traits which normally accompany them in perception, they are not, d'Alembert insists, thereby denatured. Mathematics may be the "farthest outpost to which the contemplation of properties can lead us" [d'Alembert 1963, 20-21], but it is nonetheless still anchored in the material universe of experience. For d'Alembert, mathematics is thus the most primitive of the empirical sciences. Primitive, that is, in the sense that its objects are the simplest, not in the intellectual maturity it requires.

Once the limits of analysis have been reached at magnitude and extension, the mind reverses its path and begins to reconstitute perception, property by property, by the reciprocal operation of synthesis, until it ultimately arrives at its departure point, the concrete experience itself. The successive stages along this route demarcate the subject matter of the various sciences. For example, add impenetrability and motion to the magnitude and extension of mathematics and the science of mechanics is created. All sciences study the same objects, but embrace their perceptual complexity to a greater or lesser extent.

Although the reverse operation of synthesis recaptures the familiar aspects of our experience, increased complexity can only be purchased at the cost of decreased certainty. In d'Alembert's opinion, the source of the vaunted certainty of mathematics lay not in its axioms (which offer no prospect of intellectual advance, since they merely state the obvious), but in the simplicity of its subject matter. "Simplicity" is an elusive notion, with almost as many definitions as users, but d'Alembert fortunately makes his meaning unambiguous: The degree of simplicity is synonymous with the degree of abstraction. It is at this point in d'Alembert's philosophy of mathematics that the influence of Descartes makes itself felt most strongly. Simplicity insures certainty for d'Alembert because the simplest notions are "free of clouds and easy to grasp" [d'Alembert 1796, i-ii]. By riveting the intellect upon clear and distinct ideas, errors due to confusion and faulty analogy are prevented. In a passage from the *Traité de Dynamique* (repeated almost verbatim in the Discourse), d'Alembert argued that it was just those abstract concepts commonly considered to be the most inaccessible which afforded the greatest illumination to the intellect, and conversely, "obscurity seems to take hold of our ideas to the extent that we examine more sensible properties in an object" [d'Alembert 1796, ii].

Yet d'Alembert insisted that such certainty be sacrificed to an understanding of nature as we experience it. The streamlined shadow of that experience studied by mathematics was not sufficient in itself, although d'Alembert certainly appreciated the intellectual appeal of pure mathematics pursued for its own sake. However, mathematics must be applied to other realms to

earn its intellectual keep: Mathematical abstractions "are useful only insofar as we do not limit ourselves to them" [d'Alembert 1963, 21]. While mathematics never leaves the domain of the empirical sciences entirely, the exclusive study of the two properties of extension and magnitude severely limits its scope. D'Alembert consequently annexed a whole category of the sciences, dubbed "mixed mathematics", to mathematics proper in his *System of Human Knowledge*. Once again quoting from his own *Traité de Dynamique*, d'Alembert described a continuum of certainty in the sciences as a function of relative simplicity of subject matter. The common denominator for all subdivisions of mixed mathematics (which included optics and "geometric" astronomy as well as the theory of probability) was the study of quantity as it relates to diverse objects of experience [d'Alembert 1963, 152-54].

However, d'Alembert was acutely aware of the abuses to which mixed mathematics might be subject. The practitioners of mixed mathematics must strike a balance between the degree of simplicity, and therefore abstraction, needed to achieve maximum certainty, and the degree of complexity, i.e., inclusion of experiential properties required for verisimilitude. On the one hand, the mixed mathematician must seek through abstraction the irreducible "simple ideas" from the complex ideas given by immediate sensation. On the other hand, he must guard against the Cartesian error of mistaking an empty mathematical derivation for a genuine law of nature by incorporating a modicum of experiential data into his reasoning [d'Alembert 1821a, 138; 1796, xxvii]. This latter condition demanded that the mathematician not only subject his conclusions to the test of experience, but actually begin his formulation with an appropriate number of experientially-derived assumptions.

D'Alembert's own scientific work might be viewed as a life-long exercise in mixed mathematics. All of his major works, and the vast majority of his memoirs, treat problems in applied mathematics, as their titles indicate: *Traité de Dynamique* (1743); *Traité de l'Equilibre et du Mouvement des Fluides* (1744); *Recherches sur la Précession des Equinoxes et sur la Nutation de la Terre* (1749), to name only a few. D'Alembert's bias toward applied mathematics was certainly not unusual among the mathematicians of this period. With the exception of Euler, almost all of d'Alembert's colleagues concentrated on applied mathematical problems (although these researches frequently yielded valuable results for pure mathematics as well). However, d'Alembert was exceptional in his Lockean demand that mathematics actually be modeled on, and ultimately derived from, experience. Pure mathematical researches in themselves are of limited value: Though total rigor may be admirable, it is ultimately empty. Pure mathematics deals with only a minuscule fraction of our experience; mixed mathematics, adroitly pursued [d'Alembert 1757, 628], promised a natural philosophy worthy of the name. With these

arguments, d'Alembert hoped to silence two types of critics of mathematics: those who ridiculed it for studying objects which didn't exist; and those, especially "ignorant physicists," who dismissed it as a mere intellectual game of abstract hypotheses [d'Alembert 1821a, 269].

It was in this spirit that d'Alembert approached probability theory. His persistent criticisms of conventional probability theory should not be read as an expression of skepticism concerning either the possibility or desirability of such a theory, for he was enthusiastic about both. The "art of conjecture-analysis of chance" won a place within the mixed mathematics class of the sciences of nature. In an elaboration of his encyclopedic scheme written after the *Preliminary Discourse*, d'Alembert praised this "art of conjecture" as an essential, if neglected, science which was particularly useful in those cases, so frequent in physics, medicine, and jurisprudence, in which we must act in ignorance of all the relevant facts. It is this inherent imperfection of conjectural science, a chronic ignorance, which makes it the ideal field for the skilled mixed mathematician. The probabilist must be both totally versed in rigorous demonstration and yet not restricted to it. He must be able to recognize not only the pure light of unalloyed truth, but also the gradations of more feeble rays beset by shadows. Above all, he must expose himself to a range of experience beyond "the austerity of mathematics" in order to "accustom himself to pass without difficulty from light to twilight" [d'Alembert 1821a, 155].

Conventional probability theory of d'Alembert's day was still a relatively young science. Building on the definitions and theorems advanced by Pascal, Fermat, De Moivre, Huyghens, and Jacques Bernoulli, mathematicians of the 18th century attempted to establish probability theory as a full-fledged branch of mathematics and to simultaneously enlarge the range of its applications. The treatise from which d'Alembert claimed to have learned probability theory and whose influence can be seen in his early *Encyclopédie* articles on the subject [Yamazaki 1971, 60-61]. Jacques Bernoulli's *Ars Conjectandi* (published posthumously in 1713), epitomizes the state of the theory: Bernoulli offers definitions of equiprobability and Pascalian mathematical expectation, theorems for combining and estimating probabilities (including the first limit theorem to enter the theory, the so-called law of large numbers), applications to games of chance, and philosophical reflections. The proper scope of probability theory, especially with respect to problems in the social and psychological realm, became the point of greatest interest and controversy for 18th century mathematicians concerned with the theory, d'Alembert included. Among French probabilists, attempts to extend the domain of probability theory into philosophy (especially the theory of knowledge) and the sciences of society can be found in works on the subject by Condorcet, Laplace, and

Poisson. Although these mathematicians did not, by and large share d'Alembert's misgivings regarding the foundations of probability theory, they did accept his premise that the success of new applications would depend upon the addition of new assumptions drawn from experience within the proposed area of application.

Reduced to their essentials, all of d'Alembert's criticisms of conventional probability theory concern its failure to pay adequate attention to experience in formulating its assumptions and definitions. D'Alembert subdivided probability theory into three parts which corresponded to its principal fields of application: the analysis of probability as applied to games of chance; the extension of the theory to problems of "common life," such as life expectancy, maritime insurance, and inoculation; and finally, the realm of true conjecture where mathematics had yet to make any inroads at all, in physics, history, medicine, jurisprudence, and "worldly science" (defined as the art of conducting human affairs so as to maximize commercial advantage without violating moral precepts). These divisions are also ranked in order of descending certainty. Although d'Alembert voices reservations concerning certain results of conventional probability theory as applied to games of chance, he is nonetheless sure that its subject matter is intrinsically the simplest of the theory's potential applications, since the ordinary rules of mathematical combination usually suffice to enumerate all possible cases. In problems concerning common life, however, "experience and observation alone can instruct us regarding the number of these cases, and then only approximately" [d'Alembert 1821a, 157-158].

Yet these problems still fall within the compass of mixed mathematics, as long as the facts which are to serve as "principles" or assumptions in the calculations are chosen judiciously. The third class of problems, which lacks both the requisite facts and method for mathematical treatment, does not enter into d'Alembert's critique of probability theory. D'Alembert intended his criticisms or clarifications of vaguely formulated definitions as constructive suggestions for probability theory as it then existed, not as a reproach for the many problems which had so far failed to surrender to the theory. By insisting that the simplest parts of the theory be perfected before progressing to more complex topics, d'Alembert remained true to his own dictum that the operation of synthesis must retrace the path of analysis one step at a time.

D'Alembert's critique of the results of probability theory as applied to games of chance and other physical problems appears in its earliest form (circa 1754) in the relevant articles of the *Encyclopédie*, beginning with the article on the coin game "Croix ou Pile" [2]. This article contains the most explicit arguments against conventional probability theory among the *Encyclopédie* pieces, and foreshadows many of d'Alembert's later criticisms.

In this and other memoirs on the subject, d'Alembert shows himself to be thoroughly conversant with the conventional theory. However, he is willing to challenge even the most elementary results of the theory (Yamazaki 1971, Part I) in order to dramatize his contention that the conventional theory had omitted crucial experiential elements of the problem situation. For d'Alembert, the St. Petersburg problem, as discussed by Daniel Bernoulli in the *Memoires* of the Academy of St. Petersburg [1730-1731; published 1738, Vol. 5] [3], served to highlight the flaws of conventional probability theory as it was customarily applied to games of chance and more important problems in physics. Although d'Alembert attached little or no significance to the solution of the problem itself, a rather minor one in the annals of gambling, he introduces it at the very outset of his discussion of the shortcomings of probability theory in the "Croix ou Pile" article, and returned to it over and over again in the following 25 years as the best single illustration of the conventional theory's neglect of relevant aspects of physical experience.

The problem itself involves a coin-toss game with two players, A and B. A tosses a coin: if heads (H) [4] occurs on the first toss, B pays A \$1; If H doesn't occur until the second toss, A receives \$2; the third toss, \$4; etc. According to conventional probability theory, A's expectation (and therefore the stake which A must pay B in order to play the game) is the sum:

$$\frac{1}{2} + \frac{2}{4} + \frac{4}{8} + \dots + \frac{2^{n-1}}{2^n} + \dots$$

Since the number of tosses is unspecified (the game ends only when A tosses an H), n could approach infinity, thereby making the expectation and the stake infinite as well. However, as Bernoulli and d'Alembert were both quick to point out, no sane person would pay even a small amount to play such a game. In this conflict between mathematics and common experience, d'Alembert consistently and unequivocally sided with common experience. Probability theory must explain, not dictate, prudent human action involving uncertainty.

In the "Croix ou Pile" article, d'Alembert reviewed the various solutions which had been proposed to the St. Petersburg problem. To those who claimed that the series was not truly infinite but only indefinite, d'Alembert countered with his definition of infinity as a quantity greater than any given quantity, which had served him so well in his discussion of the calculus. Moreover, even if the game were limited to some large number of tosses n for the sake of practicality, d'Alembert argued that it would still be difficult to persuade anyone but a moron to play for a stake of $n/2$.

Daniel Bernoulli's own suggested solution was based on the concept of moral expectation: A can only wager his available

capital and no more; thus the expectation and the stake remain finite. D'Alembert rejected Bernoulli's solution on two grounds. First, it does not obviate the problem that prudent people would be reluctant to play the game under almost any circumstances, much less stake their entire fortune on its outcome. Second, the notion of moral expectation, which weighs expected gains and losses against the player's available capital, had been arbitrarily simplified and arbitrarily applied. One might easily imagine other factors besides wealth regarding the personal circumstances of the players that could also be plausibly included in the problem, such as state of mind, but which are prohibitively difficult to quantify. More importantly, all other problems of mathematical expectation as ordinarily treated by probability theory take no account of any form of moral expectation, even the financial status of the players. Why d'Alembert queried, should an ad hoc exception to these rules be made for a particular problem? In a tone reminiscent of his analysis of the inconsistencies of the calculus, d'Alembert concluded his discussion of the St. Petersburg problem with an appeal to mathematicians to rid themselves of this "scandal" [d'Alembert 1757, 512-513].

The St. Petersburg problem became the leitmotif of d'Alembert's critique of conventional probability theory. In subsequent memoirs contained in his *Opuscules Mathématiques* [d'Alembert 1761, 1768, 1780], d'Alembert used this paradox as a tool with which to sharpen the contradictions he found within the accepted theory of probability, and to define the proper relations of probability theory to experience. Since the points raised in these papers overlap to a large extent, I shall discuss them jointly. D'Alembert based his analysis of the St. Petersburg problem on a three-fold distinction between possibility, plausibility ("vraisemblable"), and probability. Possibility, in this context, refers to what d'Alembert alternately described as "metaphysical" or "mathematical" possibility, while plausibility denoted physical possibility. With respect to the St. Petersburg problem, it may be metaphysically possible that the outcome of some large number n tosses with a fair coin will all be tails ($P=1/2^n$), but it is nonetheless physically implausible [d'Alembert 1780, 40]. The conventional notion had, argued d'Alembert, confused these two types of possibility, the physical and the mathematical, by failing to take adequate notice of physical experience in which a long consecutive sequence of identical events never happens purely by chance. Probabilists pay for this confusion in absurd results, like the infinite expectation computed for the St. Petersburg problem.

D'Alembert characteristically defended his exclusion of the metaphysical possibility of n -tails-in-a-row by recourse to a model of physical reality. His discussion of this model and its implications for probability theory provide a striking example of the mixed mathematician's art of compromise between

the competing demands of abstraction and verisimilitude. D'Alembert supposed that there exist n different physical ways for heads (H) and tails (T) to occur given a fair coin, thus conceding initial equiprobability to the side of simplicity. Imagine that the outcome of the first toss is T: What will be the respective probabilities for H and T on the second toss? Since nature is overwhelmingly complex, d'Alembert argued, and because there are "causes continually acting to change their state at each instant" [d'Alembert 1780, 40], once one such physical route to a T outcome has been exhausted, there remain only $n-1$ ways to obtain T, but H is still the endpoint of n possible routes. Therefore, concluded d'Alembert, H is marginally more probable ($P(H)/P(T) = n/n-1$) on the second toss. Even if n is infinite, implying that for any finite number m trials, $n-m$ still is infinite so that H and T remain equiprobable, d'Alembert contended that as m increases, it becomes ever more likely ("vraisemblable") that the next toss will be found among the sequences yet to be executed. In short, probability theory must attend to our experientially based conviction that no event in nature is ever duplicated exactly.

To emphasize the relevance of this model of nature to problems of the St. Petersburg genre, d'Alembert imagined a massive experiment in which 2^{100} people each toss a fair coin 100 times, and then compare results. D'Alembert maintained that the all -H and all -T sequences will never occur, but that some of the mixed sequences will be repeated two or more times, thus experimentally contradicting the prediction of conventional probability theory of equiprobability ($P=1/2^{100}$) for any and all sequences. By including the merely metaphysical possibility of a uniform sequence on an equal footing with more physically plausible ones, d'Alembert claimed that the St. Petersburg problem was based upon manifestly (i.e., as tested by d'Alembert's thought experiments) false premisses.

Although d'Alembert believed that conventional probability theory had erred on the side of oversimplification, he by no means despaired of salvaging the theory with suitable emendations. A large part of his critical discussion of probability theory was devoted to recasting the assumptions and the definitions of the accepted theory into alternative mathematical form. For example, d'Alembert's modified solution to the St. Petersburg problem consisted of constructing a series which would not only converge, but the value of whose terms also decreased continuously in accordance with the physical model described above. (D'Alembert rejected as simplistic Buffon's suggestion in the fourth volume of the *Supplément de l'Histoire Naturelle* that all probabilities less than 0.0001 be excluded as impossible, since it implied a discontinuous method of estimating probability values [d'Alembert 1780, 49]).

D'Alembert's series estimated the value of each successive term separately: For the first toss, $P(H)=P(T)=1/2$; for the second toss, however, $P(H)=1+a/2$ and $P(T)=1-a/2$, assuming that the outcome of the first toss was T; for the third, $P(TTH)=(1/2)(1-a/2)(1+a+b/2)$, where the limit of the sum $a+b+c\dots$ is 1 as the number of terms approaches infinity. Hence, for the St. Petersburg problem, the revised expectation would be:

$$(1/2)[1+1+a+(1-a)(1+a+b)+(1-a)(1-a-b)(1+a+b+c)+\dots].$$

Since the term $(1-a-b-c-\dots)$ ultimately approaches zero, the last factor estimating the value for an infinitely long sequence of H's or T's is also zero. D'Alembert explored other properties of the sum as well, such as relative rates of convergence [d'Alembert 1780, 58]. Note that d'Alembert here not only rejects the assumption of equiprobability but also that of the independence of consecutive coin tosses. Although d'Alembert admitted that his solution was not only cumbersome but also begged the question as to exactly how the small increments a, b, c, \dots were to be evaluated, he nonetheless maintained that this revised version of probability theory was superior to the conventional formulation because of its greater verisimilitude, leaving it to other mathematicians to develop some reliable method for assigning such probabilities [5] [d'Alembert 1780, 60].

D'Alembert defended the principle behind his experiential solution to problems of the St. Petersburg family by pointing to areas in which probability theory had been modified to fit experience with great advantage, namely, in its application to life insurance schemes. While it is metaphysically conceivable that 100 normal persons born in the same year might all die at age 30, experience contradicts this hypothesis, thus making the life insurance business a profitable concern. D'Alembert asked why, if the corrective of experience had been applied to probability in this case, should other physical situations (such as coin-tossing) be exempt? The testimony of experience is even stronger in the latter: No one has ever observed an identical event to happen many times in a row, unless there is some underlying uniform cause at work (such as a biased coin). D'Alembert's interpretation of probability theory resembles Laplace's epistemological one [d'Alembert 1780, 49; Laplace 1825, 3-4] but with a twist: For both mathematicians, probability was subjective rather than objective, a measure of our ignorance rather than any true indeterminacy in nature. D'Alembert went further, however, and urged probabilists not to exaggerate the extent of our ignorance in those areas in which experience can be consulted:

Our experience and understanding of the laws of nature teach us that the same event never happens many times in a row, and it is by virtue of this acquired knowledge

that we dismiss the repetition of "heads" or "tails" many times consecutively [d'Alembert 1780, 48; emphasis in the original].

For d'Alembert, probability theory was an outgrowth of the complexity of nature, which is such an intricate web of causes that the net effect appears chaotic to the limited human intellect. Just as the mixed mathematician must take care that his model of such situations is not belied by physical experience, so he must also be able to distinguish those cases where an intelligible cause is indeed operative and where probability theory must give way to induction. If, for example, in the hundred coin-toss experiment, one player *did* toss a hundred tails in a row, he would be justified in wagering that the outcome of the 101st toss would also be tails, since some regular cause (e.g., the construction of the coin) was evidently at work [d'Alembert 1768, 90]. In d'Alembert's opinion, Daniel Bernoulli's memoir on the origin of the solar system [Bernoulli 1784] was just such a misapplication of probability theory. Reasoning from the astronomical observation that the orbital planes of all the known planets were all clustered within a few degrees of the plane of the ecliptic, and that, moreover, their directions of revolution were all the same, Bernoulli computed the probability of such an orderly arrangement resulting solely from the action of chance to be less than 10^{-6} . Bernoulli concluded that the staggering large chance against such a coincidence demonstrated the common origin of the planets.

While d'Alembert seconded Bernoulli's conclusion of a uniform cause, and further remarked that such "coincidences" formed the basis for most fruitful scientific investigations, he protested this application of probability theory to a case where the action of a single uniform cause was already evident [d'Alembert 1821b, 459]. If Bernoulli had consistently applied conventional probability theory, he would have been forced to admit that if the actual arrangement of the solar system was enormously improbable, so was any other given arrangement (just as in the case of the hundred coin-toss experiment, in which both the all-H/all-T sequences and any given mix of H and T have the identical probability $1/2^{100}$). Since all combinations are metaphysically equiprobable (or in this case, equi-improbable), why, d'Alembert asked, search for a common cause in the one case but not in the other? D'Alembert argued that the orderly system of the planets did indeed point to a uniform cause, and that a hundred tails in a row would "also announce a cause" at work [d'Alembert 1768, 89]: Neither situation would belong to the domain of probability theory any longer.

The chance events treated by probability theory were not, in d'Alembert's mind, objectively random, but rather determined by so many distinct yet interwoven causes, or by so many differ-

ent causal routes terminating in the same effect, that no uniform, regular cause was discernible to the human observer. In such cases, mixed mathematicians could rely on experience only to set the boundaries of physical possibility. However, if these boundaries were ever overstepped by an event of "extraordinary uniformity...outside the natural order" [d'Alembert 1768, 90], the situation called for the method of induction, that is, the observation of regular causes, not the art of conjecture. Once again, acquired experience enables the mixed mathematician to make such distinctions. D'Alembert described a sequence of tiles, each imprinted with a letter of the alphabet, arranged in the combination "Constantinopolitanensibus." While someone ignorant of Latin and the existence of the city of Constantinople might regard the sequence as a result of sheer chance, a person knowing both would be convinced that the string of letters was the "work of an intelligent cause" [d'Alembert 1780, 48]. For d'Alembert, this conviction is completely analogous to that which the well-informed mixed mathematician brings to the realm of natural phenomena. Probabilistic situations become inductive ones with the accumulation of experience, and even within probability theory refinements are possible as the limits of our knowledge recede.

However intricate d'Alembert conceived the workings of the natural world to be, they hardly compared to the complexity of the moral world, or "common life." Consequently, the mixed mathematician who delves into the problems of the second branch of probability theory must be even more meticulous in matching mathematics to experience. In d'Alembert's discussions of this second part of the theory, the problem of inoculation plays much the same role that the St. Petersburg problem did for the first: It focused d'Alembert's reservations and objections on a concrete example upon which specific aspects of experience could be brought to bear. As in the case of the St. Petersburg problem, d'Alembert's point of departure was a memoir by Daniel Bernoulli, advancing a probabilistic interpretation of the relative advantages and disadvantages of smallpox inoculation [Bernoulli 1760]. Unlike the St. Petersburg problem, however, the issue of inoculation was of urgent practical interest and hotly debated throughout Europe during the period 1750-1770, especially in France, where it became a pet crusade of the *philosophes*.

In his memoir, Bernoulli argued that while the data on smallpox mortality was woefully deficient, one could still apply the theory of probability to the problem of inoculation on the assumption that "the simplest laws of nature are always the most plausible" [Bernoulli 1760, 6]. Using the incomplete mortality tables and assuming that a smallpox victim's chance of dying from the disease was a constant regardless of age, Bernoulli derived an expression for the number of people likely to succumb to smallpox in a given time period and computed the average gain

in life expectancy from inoculation for any given age. In computing life expectancies, Bernoulli employed the formula analogous to that of mathematical expectation for a lottery: the area under the curve of mortality (equivalent to the product of the number of gamblers and their stakes) divided by the number of living persons (the number of gamblers). Although Bernoulli refrained from endorsing inoculation explicitly, the favorable thrust of his analysis was unmistakable, as were the implications of his request that La Condamine, the foremost French proponent of inoculation, consider Bernoulli's arguments [Bernoulli 1760, 52-53].

In an introduction written five years after the original memoir was submitted to the Paris Academy of Sciences in 1760 and appended to the published paper in 1766, Bernoulli defended his simplifying assumptions as consistent with, if not proven by, current medical knowledge of the disease and the admittedly incomplete tables of mortality. After all, Bernoulli argued, such agreement with phenomena as far as known was the only basis for belief in the universal law of gravitation. As for those critics who missed the point of this lofty comparison, Bernoulli exhorted them to "take the trouble to apply themselves to the facts of the matters which they propose before making criticisms" [Bernoulli 1760, 3-7].

There can be little doubt that Bernoulli had d'Alembert in mind. D'Alembert had read a paper on the application of probability theory to the inoculation problem before a public session of the Academy of Sciences on November 12, 1760, which was a long and detailed critique of Bernoulli's memoir on the subject. The unidentified critic addressed in Bernoulli's introduction is described as a person of "merit and great reputation." Bernoulli and d'Alembert were lifelong rivals in mathematics [Hankins 1972, 44-50], and Bernoulli was understandably piqued to see his memoir so harshly criticized by his old opponent in a public (versus academic) address before his own paper had been published [6]. On his side, d'Alembert was stung by Bernoulli's patronizing suggestion that d'Alembert apply himself to the "facts of the matter," since d'Alembert perceived attention to the facts of experience to be at the heart of his own alternative approach to probability theory. D'Alembert retorted that he was not surprised that those ignorant of analysis had prematurely attempted to compute the advantages of inoculation, but was indeed dismayed that they should count "a man such as M. Daniel Bernoulli" as one of their number [d'Alembert 1768, ix].

Personal animosity between the two mathematicians may have intensified the dispute over the inoculation problem, but it is difficult to ascribe d'Alembert's interest in the problem wholly to pugnacity. D'Alembert's criticism of Bernoulli's solution to the inoculation problem was entirely consistent with his criticisms of conventional probability theory's treatment of physical

problems, and ultimately rested on the same belief in the primacy of (in this case, psychological) experience. Moreover, d'Alembert risked his standing as a leading *philosophe* by opposing Bernoulli on the inoculation issue, suggesting that his convictions on the subject went deeper than professional rivalry. Voltaire had advocated inoculation in a chapter of his famous *Lettres Philosophiques* (Voltaire 1734), and by 1755, inoculation had become a central part of the *philosophes'* campaign for rational social reform against the reactionary forces of superstition. At the height of the controversy, inoculation became a favorite theme in popular French literature and almost a symbol of social and intellectual liberalism (Rowbotham 1935). Although d'Alembert took great pains to endorse inoculation and to discount the theological arguments mustered against it, his critique of Bernoulli's memoir must have been viewed by his fellow *philosophes* as an attempt to undermine the strongest arguments in favor of the procedure.

D'Alembert's criticisms of Bernoulli's memoir have a familiar ring: His major objections take the conventional treatment of the problem to task for insufficient fidelity to relevant experience. The conventional method of computing life expectancies, contended d'Alembert, ignored crucial distinctions. Imagine two mortality curves *AOCD* and *AQCD*, which have identical integrals (Figure 1). According to Bernoulli's method, the average life expectancy is the same for both curves. However, d'Alembert asserts, the destinies of persons on the two curves differ significantly: *AOCD* is preferable since the number of deaths at an early age is less (d'Alembert 1768, 93). The con-

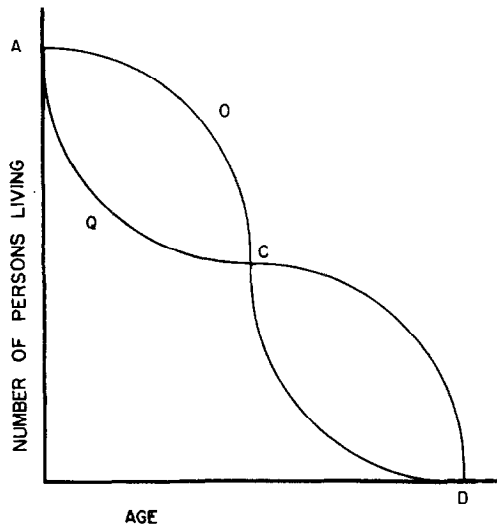


FIGURE 1

ventional formula makes no provision for factors which are critical from the standpoint of the individual.

Similarly, in the inoculation problem, d'Alembert claimed that conventional probabilistic treatment neglected elements of psychological experience which were essential for evaluating the situation mathematically. Even if Bernoulli's assumption that the risk of dying from smallpox does not vary with age (d'Alembert was highly skeptical of this postulate and called for more detailed mortality tables to settle the question) were accepted, his solution did not, d'Alembert held, fully characterize the quandary of the person facing inoculation. The missing elements might be described as quality of life considerations: Is the risk for a 30 year-old, who might expect to live 30 more years naturally and 34 more years with inoculation, comparable to that of an older person who stands to gain the same increment in life expectancy? According to d'Alembert, the 1/200 risk of dying from the inoculation itself in the prime of life must be balanced against the advantages of four more years added to the nether extreme of life, when one is less capable of enjoying them [d'Alembert 1761, 33].

For d'Alembert, the chief flaw of conventionally derived expectations was their failure to coincide with psychological expectation, i.e., "moral" experience. In this case, the gap between mathematics and reality stemmed largely from the difficulty of balancing clear and present (although small) dangers such as inoculation against a greater risk of the disease itself spread out over a longer period. The integral of the mortality curve contains no information on where the curve dips and swells, but it is just such information which is of paramount importance to the individual. D'Alembert summarized his objections to Bernoulli's method with an analogy drawn from the first branch of probability theory. He described a lottery in which all citizens were required to participate. After the drawing, half of the participants would immediately be put to death, while fortunate remainder would be guaranteed a lifespan of a hundred years. By conventional estimation, the average life expectancy of the entire population of newborns would be 50 years. Yet d'Alembert doubted whether anyone would voluntarily participate in such a lottery, even if the average life expectancy might ordinarily be less than 50 years.

It is important to note that d'Alembert does not condone this form of reasoning which prefers large longterm risks to smaller, more immediate ones, which he called the "common logic...half good, half bad" [d'Alembert 1761, 35]. However, he did despair of changing it. As in the case of the St. Petersburg problem, mixed mathematics must follow, not lead, common experience--in the inoculation problem, the actual psychology of risk-taking. Mathematics is descriptive, not normative [7]. On a metaphysical plane, the conventional theory of probability could not be

faulted for its treatment of either the inoculation or St. Petersburg problems. Yet both treatments, d'Alembert maintained, were irrelevant to our actual experience of the physical and moral worlds.

D'Alembert deepened his critique of Bernoulli's solution of the inoculation problem by including still other aspects of moral experience. The advantages of inoculation must be judged with respect to three different types of life duration, according to d'Alembert: physical life or ordinary duration (the only type recognized in the accepted theory); "real life" or that portion of physical life which is lived fully, without suffering; and "civil life" or that portion of physical life in which one is useful to the state [d'Alembert 1761, 82-88]. Depending on the vantage point chosen, the relative benefit to be derived from inoculation would vary widely, as d'Alembert demonstrated by constructing mortality curves for real and civil lifetimes. The slope of real life curves varied as a function of enjoyment, leveling off both in early childhood and old age; the curve of civil life dipped below the abscissa to represent those periods during which the individual is a ward of the state and therefore of negative utility. While d'Alembert was sensitive to the difficulties of such estimations, which were certain to fluctuate with individual circumstance, he nonetheless insisted that a sound probabilistic treatment of moral problems like inoculation could not afford to ignore them. D'Alembert's list of requirements for a valid mathematical theory of inoculation was an appeal to experience at many levels: accurate mortality tables; a more sensitive method for computing life expectancies; a mathematical way of weighing small short-term risks against large long-term ones; a theory for comparing physical, real, and civil lifetimes.

D'Alembert's critique of probability theory won few converts, although several of his contemporaries, notably Condorcet and Laplace, tacitly acknowledged the force of his arguments. While Condorcet never mentioned d'Alembert by name in any of his published probabilistic writings, his interest in the subject dates from an unpublished rebuttal defending d'Alembert's views on probability against Massé de la Rudelière's *Défense de la Doctrine des Combinaisons...* (1763) [Baker 1975, 176-177] and Daniel Bernoulli's tracts. A straight line can be drawn from this draft [Condorcet, Ms 883] in which Condorcet wrestles with "a common measure that one can take in the calculus of probabilities," to his six-part early memoir on probability theory submitted to the Academy of Sciences between 1784 and 1787. D'Alembert's influence is most clearly shown in Condorcet's choice of problems, which address d'Alembert's gravest objections to the conventional theory. Condorcet's most notable effort in response to d'Alembert's critique was an attempt to redefine the Pascalian concept of mathematical expectation in terms of a "mean" rather than

"real" value [Condorcet 1784].

Condorcet's frequently voiced philosophical reservations concerning the nature of the results obtained by means of probability theory also reveal his debt to d'Alembert. In an early draft [Baker 1975, 178], Condorcet echoes d'Alembert's distinction between an "abstract and metaphysical possibility" and a "physical and real" one, and offers an example (significantly of the St. Petersburg type) to demonstrate that three equally valid methods result in three conflicting probability values for the same situation [Condorcet, Ms 875, 93-94]. This early distinction borrowed from d'Alembert formed the basis for Condorcet's later dichotomy between absolute, mathematical probabilities and "motivations for belief," the latter being based on actual experience and therefore dictating conduct [Condorcet 1805, 90; Granger 1956, 147]. In his academic eulogy of d'Alembert and only published commentary on the latter's critique of probability theory, Condorcet assessed d'Alembert's contribution as a fundamental one:

...if this calculus of probabilities one day rests
on more certain foundations, it will be to M. d'Alembert
that we will be obliged [Condorcet 1847, 92].

D'Alembert's influence on Laplace and other contemporary mathematicians was far more diffuse and is therefore more difficult to interpret. Laplace's 1774 solution to the St. Petersburg problem, which postulated a physical inequality in the coin on the grounds that "the science of chance must be employed with caution, and must be modified when one passes from the mathematical to the physical case" [Laplace 1774], was very much in the spirit of d'Alembert's critique and enthusiastically received in the latter's *Opuscles* [d'Alembert 1780, 60]. However, Laplace's 1812 solution in the *Théorie Analytique des Probabilités* reverted to Daniel Bernoulli's moral expectation. Yet his reluctance to quantify the "infinity of circumstances particular to each individual which are impossible to evaluate" [Laplace 1847, 482] is reminiscent of d'Alembert's earlier reluctance to embrace the notion of moral expectation.

With the exception of Daniel Bernoulli, none of d'Alembert's contemporaries actually rejected his critique outright. Montucla, however, after describing d'Alembert's objections and judging them "plausible," observed that "they have not unsettled the generally accepted theory of probability in the minds of mathematicians in general" [Montucla 1802, 406]. The complexity which d'Alembert himself recognized as the major obstacle to the acceptance of his alternative formulations overshadowed the insights they contributed. D'Alembert offered many tentative reworkings of definitions and formalisms, but remained unsatisfied with all of them. In the work of Laplace, early 19th century probability theory continued on the path marked out by the ortho-

dox 18th century works of Jacques and Daniel Bernoulli. Although many concepts of 20th century decision theory resemble d'Alembert's concern with quantifying quality-of-life factors, the historical links between the two are tenuous or nonexistent.

D'Alembert's criticisms have prompted a range of reactions from historians of probability theory. Some have dismissed them as absurd [Todhunter 1949, 258]; others have treated them as a curiosity [Maistrov 1974, 127-128]. A recent analysis has more sympathetically attempted to discover an internal, if faulty, logic to d'Alembert's critique, or to at least supply some consistent rationale for his objections [Yamazaki 1971, 60]. Attempts to understand d'Alembert's "eccentric" stance on probability theory within the broader context of his scientific work have done so by emphasizing rationalist tendencies which would have made the very notion of a "science of uncertainty" suspect [Hankins 1972, 142-149]. This interpretation is belied by d'Alembert's own protest that he wished only to "amend and improve" the conventional theory. I have argued here that, far from calling for greater deductive rigor, d'Alembert's critique attacked the accepted theory's excessive reliance on the abstract, purely mathematical notions which he believed to offer certainty in human reasoning by screening out experience.

Within the sphere of d'Alembert's philosophy of mathematics, his criticisms of probability theory are wholly intelligible. Committed to a mathematics which was experiential both in its origins and ultimate aims, d'Alembert sought to reconcile the claims of simplicity and verisimilitude in the two divisions of probability theory belonging to mixed mathematics. In doing so, he trusted his "*esprit géométrique*" [d'Alembert 1757] to set the optimal ratio of experience to abstraction, faulting conventional probability theory for its neglect of the former. D'Alembert's alternative formulations were thus part of the same mathematical program which proved so successful when applied to the calculus: to achieve a closer match between mathematics and experience [8]. For d'Alembert, pure mathematics was not the culmination but only the starting point of natural philosophy. Mixed mathematics represented the first steps back to experience along the route of synthesis. Hence, it is not surprising that most of d'Alembert's scientific works subordinated pure to applied mathematics, and that d'Alembert, along with the majority of his contemporaries, regarded the boundary between the two disciplines as a fluid one. In the case of probability theory, d'Alembert's allegiance to physical and psychological experience exacted too high a price in complexity. Modern probability theory has solved d'Alembert's dilemma by enforcing a three-fold division which divorces the pure mathematical theory from its application and both of these from ordinary experience [Feller 1957, 1-3]. For d'Alembert, an 18th century mathematician and *philosophe* steeped in the sensationalist philosophies of Locke and Condillac, however, mathematics remained the original empirical science.

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NOTES

1. All translations are my own unless otherwise indicated.
2. *Encyclopédie* articles by d'Alembert relating to probability theory include: "Absent," "Avantage," "Bassette," "Carreau," "Combinaison," "Croix ou Pile," "Dé," "Loterie," and "Paré"; and in the *Supplement*, "Problème sur les cartes." The article on "Probabilité" was evidently written by Diderot.
3. The problem was first proposed by Nikolaus Bernoulli I in a letter to Montmort, published in the latter's 1713 edition of *Essai d'Analyse sur les Jeux de Hasard*.
4. For the sake of convenience, I will translate "croix" as "heads"; "pile" as "tails"; and all currency as dollars.
5. D'Alembert refers interested readers to Bayes' paper in the *Philosophical Transactions* (1763, published 1764) as communicated by Price, and to Laplace's discussion of probabilities for a biased coin [Laplace 1774].
6. Bernoulli in fact complained of shabby treatment, particularly in respect to publishing priority, to the Paris Academy of Sciences. D'Alembert replied at the meeting of December 7, 1762 (Archives de l'Académie des Sciences, Dossier 7 décembre 1762), rebuking Bernoulli for his "injurious manner," and for having erected "great calculations on vague hypotheses in a matter concerning human life."
7. Condorcet also believed that the results of probability theory should not contradict what "the simplest reasoning would have dictated" (Condorcet 1785, ii), but rather only clarify cases in which common sense ("bon sens") might be confused by sophistries.
8. D'Alembert's reassessment of the foundations of the calculus in terms of the limit concept stemmed from his insistence that mathematical concepts both originate in experience and be mentally clear. A concept like infinity, as applied either to very large or very small magnitudes, had no genuine roots in experience and thus provided no departure point for the simplifying process of abstraction to work from. The in-

infinitesimals of the calculus therefore "do not exist really either in nature, nor in the suppositions of mathematicians" and must be replaced with limit notions smaller than any given magnitude [d'Alembert 1789, 208].

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